Short-run subsidies, take-up, and long-run demand for off-grid solar for the poor: Evidence from large-scale randomized trials in Rwanda

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A dissertation submitted for the degree of Master of Commerce in Economics

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Abstract

More than a billion people lack access to modern electricity and instead rely on kerosene and other dirty lighting sources, grid expansion is not expected to keep pace with population growth, and both contribute to climate change. Moreover, pneumonia is the leading cause of death for under-fives in the world and kerosene smoke is a significant risk factor. For-profit distribution of low-cost solar LEDs has been touted as an answer, but adoption remains low, especially by the poorest. This study estimates demand curves for both the initial price of low-cost solar LEDs as well as the subsequent user fee for repeated purchases, while also estimating the impact of short-run subsidies, or a free trial period, on long-run demand. We find uptake is highly sensitive to price with most households purchasing at zero price and none at full cost. Furthermore, using unique objective big data on long-term usage we show that households that received lights for free use their lights as much as those that paid a positive price, disproving the notion, in this context, that consumers will not use goods they received for free. Finally, we find short-term subsidies for user fees actually increases long-term demand in the context of repeated purchases.

[1] Acknowledgements. I wish to especially thank my supervisor, Prof Martine Visser, who has provided invaluable support and guidance throughout this project. I would also like to thank Manuel Barron, Bhavani Shanker Uppari, Ioana Popescu, and Serguei Netessine as well as our partners at Nuru Energy, including Sameer Hajee, Simon Tremeer, and Barry Whitmill and Innovations for Poverty Action including Kris Cox, Phillip Okull, Sandra Nkusi, and Emmanuel Kwizera.

For funding I gratefully thank ENERGIA, and the UK Department for International Development (DFID). I also thank Grand Challenges Canada, Stars in Global Health, as well as the International Growth Centre, INSEAD, and the Wharton School of Business, University of Pennsylvania, and Environment for Development, for additional funding.

Executive Summary

More than a billion people lack access to modern electricity and instead rely on dirty lighting sources, while grid expansion is not expected to keep pace with population growth. Almost 6 million children under five die each year from preventable diseases, while respiratory infection is the leading cause and indoor air pollution from traditional lighting is a risk factor. For-profit distribution of low-cost solar LED lights has been touted as an answer, but even with large investments adoption remains low, especially by the poorest. This dissertation presents results from five key research questions including two large-scale randomized controlled trials in rural Rwanda which *i)* experimentally estimate demand curves for the initial price of low-cost LEDs, *ii)* studies how long-run usage varies with initial price paid, *iii)* experimentally estimate demand curves for long-run pay-as-you-go (PAYG) user fees, and finally *iv)* evaluates the impact of short-run subsidies for user fees, or a free trial intervention, on long-run demand.

We find take-up is highly sensitive to price with most households purchasing at zero and few at full cost. Furthermore, using unique big data on objectively measured long-term usage we show that households that received lights for free use them as much as those that paid a positive price, disproving, in this context, the notion that people don't use goods they received for free. The policy result is that it is possible to charge zero initial prices and smooth costs over time with micro user fees. However, we show experimentally that even increasing user fees from zero reduces long-run demand substantially. Taken together, these results provide strong evidence for subsidies if the aim is to reach full adoption by the ultra-poor, or the very least a reduced pricing strategy that makes exclusive use of PAYG user fees if a sustainable for-profit model is a necessity.

Given this reality what else can development actors do to increase the low adoption rates of their products amongst the extreme poor? Recent behavioral science research shows that incentive based behavioral interventions can work. Most relevant is that they can have effects that last beyond the treatment period. We implement such an intervention in the form of a free trial where user fees are set to zero for a period of three months. Free trials or short-run subsidies for solar lights can be considered a price incentive aimed at cementing a new welfare improving and emissions reducing habit. It can also be thought of as an intervention with the goal of increasing positive learning: a free trial can remove information frictions because it allows uninformed consumers to use the product and learn its benefits over existing alternatives, which leads to higher use and willingness to pay in the long-run. Finally, short-run subsidies can also backfire if people anchor on the initial low or zero price and are subsequently unwilling to pay full prices later. The nascent literature on products for the poor finds a large role for both price anchoring and positive learning (the latter via the alleviation of binding information constraints). In our context, we find short-term subsidies for user fees successfully increases long-term demand even after the removal of subsidies. Therefore, positive factors such as learning and habit formation outweigh negative factors including price anchoring.

JEL: D11, D12, D83, I11, I18, O12

Keywords: subsidies; health; pricing; learning, energy, information frictions, behavioral economics

1 Introduction

More than a billion people worldwide lack access to basic electricity. In Africa alone 600 million remain off-grid, with this projected to rise to 700 given the inability of grid expansion to keep pace with population growth (IEA, 2016; World Bank, 2018;). In Rwanda as of 2018, 12 percent of rural households are electrified, with most earning less than $1.25 per day and relying on dim, expensive, polluting, and harmful kerosene lamps, flashlights, fires or even simple sticks for their lighting needs (USAID, 2018). Moreover, Rwanda has higher rates of grid connections—and an impressive electrification program—than most central and east African countries where electrification rates are the lowest in the world (IEA, 2013; World Bank, 2018). Even where the grid is available, however, most rural households, are unable to afford the typically steep connection fees, so that on paper it looks like electricity access is high when it is far from it (Lee et al., 2016b).[2]

Respiratory infections are the leading cause of death for under-fives in the world and almost 6 million children under the age of five die per year (Liu et al., 2016). Most of these deaths are preventable using simple health technologies such as solar lights, insecticide-treated bednets, water purification, oral rehydration, and antibiotics (UNICEF, 2018). Moreover, a robust relationship between air pollution and infant mortality in Africa has recently been demonstrated (Burke et al., 2018), and experimental evidence from El Salvador indicates kerosene smoke has a significant impact on indoor air pollution (IAP) and child respiratory health (Barron and Torero, 2017)[3]. Finally, air pollution from coal-powered grid electricity, or kerosene, fires, and candles, contributes significantly to global warming. Clearly, solving the challenges of electrification and IAP is urgent in order to prevent further loss of life and negative climate change impacts. Yet the question of how to raise adoption rates remains unanswered and, recent evidence from a large field

[2] A typical connection fee for a rural household can be as high as $400 USD. Large-scale data gathering has confirmed most poor rural households are ‘under grid’, that is grid connections are available nearby but households’ have been unable to connect, see for example Lee et al., 2016b

[3] It is perhaps not surprising given households cook outside of the main dwelling in El Salvador, where Barron and Torero (2017) took place. The situation is the same in Rwanda where the vast majority of households cook outdoors. Cooking is generally undertaken by the female head of the household in a separate cooking area. It makes sense then that, even though a cook-fire emits more pollutants, lights, often multiple lights used continuously indoors, might contribute more to indoor air pollution and thus child respiratory health.

experiment suggests rural grid electricity may be welfare reducing given the high costs involved (Lee et al., 2016a).

A proposed solution is for-profit distribution of low-cost off-grid solar powered LED lights and home-solar-systems as promulgated by the World Bank and other international organizations. Indeed, the International Energy Agency's projections assume that 70% of all rural households will have to rely on micro-grid or low-cost off-grid solutions given the high costs of rural electrification (IEA, 2012). Yet, in the context of significant investment, adoption of these technologies remains low and there is small-scale evidence many poor households are unable to afford even the lowest cost solar lanterns (Grimm et al, 2016b; in one example program, the UK and US alone, via Power Africa, recently committed $1 billion to off-grid and small-scale solar solutions in Sub-Saharan Africa[4]).

How can firms and policymakers increase adoption? Can social businesses drive demand increases or should governments and international NGOs distribute at subsidized rates? What are the most effective pricing strategies to do so? If subsidies, or a reduced pricing pay-as-you-go (PAYG) strategy, are required how do one-off short-run subsidies affect both short-run adoption as well as long-run demand of this new and important environmental health technology, solar LEDs? Using multiple large-scale randomized controlled trials and unique big data from rural Rwanda, we add to this debate by evaluating different models (in terms of upfront pricing, repeated user fees, and a free trial, or short-run subsidy, intervention) for effectively scaling up the distribution of affordable renewable lighting to the rural poor in developing countries. We partner with a social business focused on providing low-cost off-grid solar light solutions to the ultra-poor in Sub-Saharan Africa with large operations in over 1500 villages in Rwanda. Each village has a village microenterprise which sells single rechargeable LED lights to their community and provides a centralized solar-powered recharge service for a small fee (including mobile phones and radios). This is an innovative approach because, by centralizing power distribution, it lowers the costs per light and is also a natural pay-as-you-go model with payments directly related to use. The pricing model depends primarily on two key factors: the upfront price of lights, as well as the level of the recharge fee. We therefore estimate demand curves, and create exogenous variation along both dimensions, allowing us to experimentally determine whether and how price affects initial take-up and how long-run demand or usage depends on initial price paid (for example, do

[4] https://www.usaid.gov/powerafrica

households' that paid a zero price use the product?). Furthermore, LEDs can be expected to be an experience good given they are of significantly higher quality than alternatives and are not widely available in rural villages leading to the existence of information frictions—whereby a lack of knowledge of product quality and benefits leads to low investment levels. There is thus scope for positive learning and new habit formation, and motivated by this we implement a short-run subsidy, or free trial intervention, and determine whether these factors can outweigh the effects of price anchoring—where consumers anchor on the initial low price and are unwilling to pay higher prices later.

To answer these questions, we gather rich data from multiple sources. We develop unique 'internet of things' remote big data capturing technology which includes the recharge information for each light transmitted automatically at every recharge. Thus, our data measure objective usage rates instead of relying on estimates from enumerator teams observing usage (as is the norm), which substantially increases the richness and accuracy of our results. Indeed, being observed frequently leads to bias—or Hawthorne effects—and even just the act of being surveyed can change behavior and related parameter estimates, via factors such as social desirability bias, where study participants give the answers they expect the survey team wants to hear (Zwane et al., 2011). Surveys are also expensive, limiting sample size and the number of treatments as well as the speed of results, a crucial factor for many for-profits. We combine this with rich administrative data, smaller scale household surveys, and GPS data to give us a clearer picture of behavioral responses. Rationale for our experiments is also data-driven. First, we conducted an observational analysis of our partner's operations in over 1000 rural villages in Rwanda and Kenya. Upfront price was varied longitudinally, not experimentally, and the differences in demand measured. The evidence suggests high price sensitivity. Second, we carried out qualitative work, a pilot, and smaller scale experiments, before finally launching two large randomized control trials.

In the first field experiment, a sample of 1987 households were randomly assigned discount vouchers for the upfront price of lights. In line with the literature on other development and health products, we find demand is highly elastic: charging 3000RWF ($10 PPP) reduces demand by 88 percentage points relative to charging 0. We next examine whether households that paid a zero, or reduced, upfront price use their lights less. Regressing initial price paid on long-run usage rates we find no effect of price paid on usage, which is contrary to expectations if the assumption is prices act as a mechanism to screen out those less likely to use the product. This also provides a rationale for subsidies—even if products are distributed for free consumers will still use them. In the second field experiment, in a sample of 2867 households, we first distribute lights for free to eliminate selection and take-up issues, and then randomly assign recharge coupons which vary the repeated PAYG user fee, from zero to 120RWF. We examine the effect of the user fee on short-run usage and find charging a zero user fee increases usage by 156 percent while charging 50RWF increases usage by 67 percent, and we conclude that long-term usage, like initial take-up, is price elastic. Therefore, even charging low rates, such as USD$0.15-0.30 PPP per recharge, will reduce adoption and use substantially, making subsidies or reduced pricing necessary if full adoption is to be achieved

Next, building on the development economics literature on pricing products for the poor (e.g., Cohen and Dupas, 2010; Dupas, 2014; Fischer et al., 2016), and the literatures on incentive-based behavioral interventions and the economic theory of habit formation (e.g., Becker and Murphy, 1988; Mochon et al., 2017), we develop an intervention involving a free trial, or short-run subsidies, for a period of three months. We estimate the effect of this treatment and find that long-run usage, is 133 percent higher for those that initially paid a zero user fee (at both three and six months after the initial three-month treatment period has elapsed, for a total study length of six to nine months). Therefore, we conclude that short-run subsidies, or a free trial period, increases long-run demand and thus habit formation and positive learning outweigh any price anchoring effects. Lastly, we carry out follow-up surveys to interrogate the internal validity of the experiments, and gathering data from multiple sources, conduct robustness tests using a variety of modeling methods and assumptions. For example, other than price, an important driver of usage may be the inconvenience cost of traveling to the village-level solar recharge center. Therefore, we gather Global Positioning Systems (GPS) data and compute the distance of each household to this recharge center to control for this potential driver of usage and increase the precision and validity of the price estimates, as well as check for randomization balance along this important correlate of use.

We make contributions in terms of policy relevant findings, both in the off-grid solar sector, and more broadly in pricing products to ensure adoption by the poor. Indeed, we directly influence policy in that our partner has implemented, at scale, the proposals flowing from this research. We also contribute to the literature in several ways. First, very little rigorous evidence exists on energy demand in developing countries even in the context of massive investments in grid as well as low-cost solar (Lee et al., 2016a). By estimating demand curves, via randomization, for both the upfront price of lights as well as the user recharge fee, we contribute valuable and much-needed rigorous evidence to this debate. Second, we add to the hotly contested debate on subsidies and pricing of new development products in developing countries by extending the results to a different but important context: a new environmental and health product—off-grid solar LEDs (Kremer & Miguel, 2007; Cohen & Dupas, 2010; Dupas, 2014a; Fischer et al., 2016). Third, the two papers which study the impact of short-run subsidies on long-run demand do not focus on more than two purchases (Dupas, 2014a; Fischer et al., 2016). Instead these studies have two rounds: an initial wave with zero or heavily subsidized prices and a subsequent wave with a uniform market price where additional products are offered for sale. We extend this literature on pricing health products to the case of repeated purchases: in our context households need to pay a PAYG user fee at least once per month in order to charge their lights and we study usage over an extended period. Because only one product is used, but repeatedly paid for, we bypass the problem that having stock on hand might reduce the probability of a second purchase, a limitation of prior research. Fourth, we contribute by collecting multiple types and sources of big data, including remote behavioral monitoring and GPS data, and combine this with household surveys. Moreover, we study actual usage rates using unique data and not those observed by survey teams as has previously been the case. Fifth, we contribute to the growing literatures which test behavioral economics and marketing in the field as well as the literature on incentive-based behavioral interventions (Mochon et al., 2017). Sixth, our experiments also consist of rigorous evaluation of business models at the 'base of the pyramid' through the use of business strategy experiments, and we make a valuable contribution to this nascent business literature.

The rest of this paper is organized as follows. Section 2 briefly outlines the related literature and background. Section 3 then examines the experimental designs and data. Section 4 details the empirical results from field experiments one and two. Section 5 discusses the findings and concludes.

2 Background and related literature

Understanding models for the effective distribution of renewable lighting to the ultra-poor is crucial given evidence on the effects of electrification on health, shifts in studying patterns, increased labor supply and productivity in housework, and more convenience during leisure activities (Dinkelman, 2011; Furukawa, 2012; Lipscomb, Mobarak & Barham, 2013; Sovacool & Drupady, 2016; Grimm et al., 2016a; Barron and Torero, 2017). However, newer evidence suggests smaller impacts with grid electricity potentially decreasing welfare given the high costs faced by rural consumers (Lee et al., 2016a). A low-cost solution which displaces dirty lighting is then essential to fill this gap. We show that 52 percent of households in Huye District in Rwanda, where a pilot welfare study was conducted (and a portion of this paper's pricing experiments took place), use a dirty lighting source, predominantly kerosene but also sticks, fire, and candles. Other households use either nothing or cheap low-quality small dry-battery flashlights. Evidence from the pilot suggests the above are replaced with solar rechargeable lights when these are made available.

For these reasons the World Bank, through its private sector investment arm the International Finance Corporation (IFC), has actively pushed the idea of for-profit distribution of low-cost solar lanterns and larger home-solar systems via its Lighting Africa initiative for almost a decade (Lighting Africa, 2012). However, the major challenge facing social enterprises has been how to profitably and sustainably distribute renewable and reliable off-grid lighting to the rural poor which eliminates kerosene. This is most likely because preliminary evidence suggests current models are still too expensive for the rural poor, and where they are successful, they do not reach the ultra-poor (Wong, 2012; Grimm et al., 2016a). How then to proceed? Are short-run subsidies, or a reduced pricing strategy, required and if so, will they be effective in the long-run?

Given subsidies are common, but controversial, tools used by policymakers to overcome the poor's credit and liquidity constraints, this is an important and contested question in the realm of products for the poor. Indeed, a frequent concern in development policy is that once-off subsidies might reduce long-term demand via reference-dependence preferences—also known as price anchoring (Dupas, 2014a). People could anchor on the subsidized price and not be willing to pay the full price later (Köszegi and Rabin, 2006; Simonsohn and Loewenstein, 2006; Dupas, 2014a). The situation is more complex when there is scope for positive learning—i.e., if a free trial gives users a chance to learn about the positive benefits of a product. We call the good an experience good if information frictions exist and households have previously underestimated the good's value. In this case it is possible that short-term subsidies, or a reduced pricing strategy, could increase long-term demand via the elimination of information frictions and resultant positive learning because consumers come to value the product more which makes them willing to pay a higher price (Fischer et al., 2016). Yet, for learning to have a positive effect on subsequent demand consumers must use the product. Many argue that if products are given away for free households will not use them reducing the screening effect of prices—where only households which will use a good are willing to pay a positive price for it (Cohen and Dupas, 2010; Ashraf, Berry and Shapiro, 2010; Chassang, Padro i Miquel and Snowberg, 2012).

Other than removal of information constraints and positive learning, another factor which could counteract the effects of price anchors is habit formation: consumers could become used to an increased amount of a product and this behavior could decay slowly over time even when the subsidy period is over. Indeed, a behavior can rationally persist when past consumption levels affect current consumption as in the canonical economic model of habit formation in the context of addiction (Becker and Murphy, 1988).

There exists an extensive literature on how non-budget constraint factors affect demand. The marketing, psychology, and economics literature finds a large role for price anchors: price histories play a significant role on subsequent demand such that lower initial prices are anchored on making consumers unwilling to pay higher prices later. The evidence from this literature draws primarily from lab experiments but also supermarket scanning data (Kalyanaram and Little, 1994, Fischer et al., 2016). In contrast, evidence from development field experiments largely finds no role for these non-budget constraint effects on prices. For example, Cohen and Dupas (2010) show, in the context of insecticide treated bednets, that demand is highly price elastic: charging even a small positive price reduces demand substantially but does not lead to increased use. They, therefore, find no role for screening effects, where consumers use a good more because they have paid for it. Kremer and Miguel (2007) find similar results in the context of deworming pills in Kenya: charging even a small price dramatically reduces demand. Moreover, Dupas (2014a), again in the context of antimalarial bednets, finds that short-run subsidies actually increase long-run demand through positive learning about the value of the product—such that a lower price today increases later demand at full price. In this case the positive learning effect outweighs the negative effect of price anchoring. This evidence from development field experiments led to a "loose policy consensus" on the free distribution of health products given: *i*) very high subsidies are necessary to increase initial adoption, *ii)* households still use goods they paid low or zero prices for and *iii)* short-term subsidies actually raise long-term demand (JPAL, 2011; Dupas, 2014b; Fischer et al., 2016). The conclusion is subsidies, or reduced prices, are required to effectively reach the poor.

This consensus, however, is no longer watertight. For instance, one early study which did not fit the body of evidence, Ashraf et al, (2010) in the context of water chlorination, found prices play a screening role such that higher initial prices stimulate subsequent use, while most significantly Fischer et al. (2016) find a large role for price anchors: Free distribution of medication lowers long-term demand consistent with the predictions of models of reference-dependent preferences (Simonsohn and Loewenstein, 2006; Köszegi and Rabin, 2006; Mazar et al., 2013; Heidhues and Koszegi, 2014**;** Fischer et al., 2016). Indeed, given how results are frequently contradictory, the authors call for more research and replication, in different contexts, and for exploring other important drivers (Fischer et al., 2016).

This study is motivated by this literature and aims to add rigorous evidence to this debate. This research is also linked to a separate older (although not experimental) literature on safe water and sanitation adoption, which frequently uses contingent valuation (CV) surveys to estimate willingness to pay (e.g., Whittington, 1998; Davis et al., 1998). Whittington (1998) surveys the use of CV surveys in developing countries and notes that the methodology can be controversial, at least in developed country academia. Finally, there is a newer experimental literature on the pricing and adoption of water, sanitation, improved cookstoves, and grid electricity which our research also contributes to (e.g., Ruiz-Mercado, et al., 2011; Mobarak et al., 2012; Lee et al., 2016a).

3 Experimental design and data

3.1 Experimental Designs

This project includes two large-scale randomized field experiments in a sample of over 5000 households, each with two different data collection methods and timespans; the short-run, and long-run. Both randomized trials were carried out in rural Rwanda; the first, Phase I, focusing on exogenously varying the upfront price of lights and the second, Phase II, implementing pricing and behavioral interventions varying the long-run PAYG user fee.

During the first randomized control trial, Phase I, lights are sold to households which is the initial stage in the business model. Here we randomize the upfront price faced by consumers in the initial take-up of lights as well as record short-run data on demand and long-run data on usage. The goal is to *i)* determine the optimal upfront pricing policy and *ii)* to study the impact of initial price paid on subsequent long-term paid usage.

The second separate randomized control trial we call Phase II. Here households receive zero upfront prices but instead face randomized PAYG user fees for longer term use. Phase II also consists of 3 months (short-run) during which we run pricing interventions, and long-run (6 months total), or an additional 3 months after pricing is reverted to business-as-usual. Here our goals are *iii*) to determine the optimal level for PAYG user fees for the short-run as well as *iv*) the effects of providing a free trial and short-term subsidies on long-term demand after the removal of subsidies.

For all interventions, we conducted initial power calculations using pre-existing pricing data from our partner's operations as well as the literature to inform the size of our treatment arms (in particular Dupas, 2014a; and Fischer et al., 2016). The power calculations were largely successful given virtually every price treatment arm is statistically significant.

2.2.1 Phase I: Randomization of the upfront price of lights

The Phase I experiments were conducted in rural Rwanda in Huye and Ruhango districts –districts broadly representative of rural Rwanda and East Africa in general. We present household socioeconomic summary statistics in Table A4 in the appendix. Following the methodology of Cohen and Dupas (2010), Dupas (2014a), and Meredith et al, (2013), a sample of 1987 households from 18 villages were randomly assigned discount vouchers for the upfront price of lights. Phase I started with 6 villages from Huye in January 2016 and 12 villages from Ruhango in January 2017. Villages were selected according to our partner's business model as follows: rural, no grid connection, no grid connection planned, and at least 90 households.

First, a list of all households in each village was obtained. Thereafter, randomization was done at the household level and stratified at the village level. Randomization of price was implemented using discount vouchers which were handed out door-to-door by trained enumerators. Vouchers were later redeemed for lights at the village level in the presence of our partner and study staff. Following the business model of our partner which entails pre-selling lights meant that we had to make the vouchers valid for only a few days. Thus, we measure the contemporaneous demand for lights.

There were nine prices and vouchers ranged in discount from a 100% subsidy, 0 price, to a 33% subsidy, 3000 Rwandan Francs or $10 PPP (at the time of the study $1 equaled approximately 750 RWF in nominal exchange rates so 3000 is roughly $4). Note this was still lower than the manufacturing cost of lights. Prices included 0, 200, 300, 500, 800, 1000, 1500, 2000, and 3000 RWF. Vouchers were printed with the actual price faced by consumers as well as the size of the discount and the market price of lights. The market price advertised however was 3000RWF as observational analysis using data on 1000 villages had already determined that the actual market price of 4500RWF was too high. Additionally, we track long-run usage of lights to see how this is related to the upfront price paid.

2.2.2 Phase II: Randomization of the long-run user fee

The Phase II randomized field experiments focused on randomly varying the micro user, or recharge, fee faced by customers. In order to do so households received a light for free and instead faced randomly differing user fees ranging from 0 to 120RWF. The experiments were carried out in Ruhango district in rural Rwanda. This component of the study contained a sample of 2867 households and lights (one light per household) from a total of 29 villages or enumerator areas.

First, a list of households in each village was obtained. Then, coupon-light combinations were randomly distributed in set proportions within each village, i.e. stratification was done at the village level in order to achieve greater balance. There were around 9 households per village per treatment arm, and seven price treatments (plus three additional battery treatments not studied here). The control coupon or reference category allowed households to recharge their lights for 100RWF per recharge (the status quo business model), while the other discount coupons included the following prices: 0, 50, 60, 70, 80, and 120 RWF.

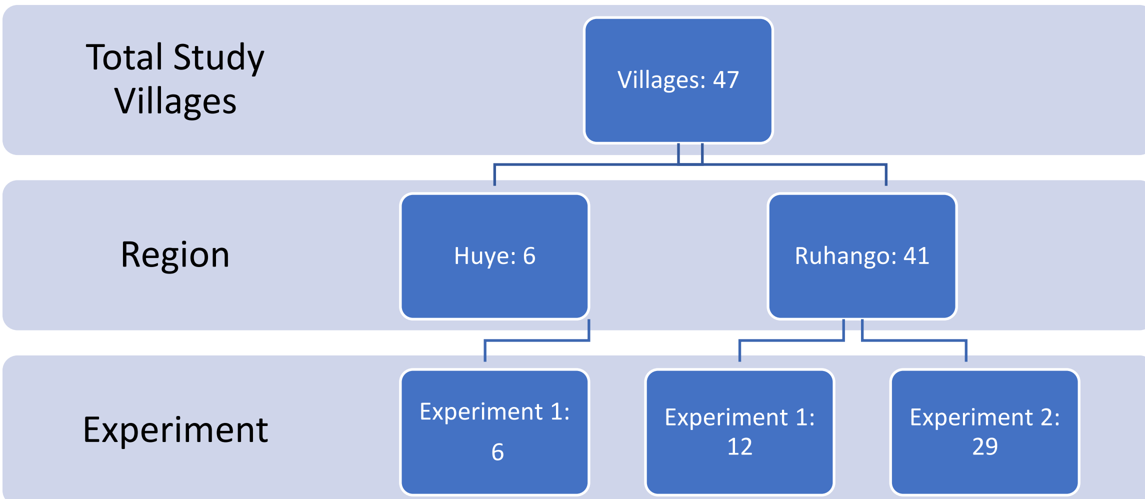
Random assignment of the interventions was achieved using recharge coupons which were redeemable at the village level microenterprise at each recharge. The coupon system was designed to operationalize the price randomization and to ensure the internal validity of the experiments by limiting potential arbitrage given we were concerned lights with low priced recharge coupons may be sold to other households. We believe this system limited arbitrage and ensured the internal validity of our experiments. It should also be noted that each household received only one light and explicitly put their name down as wanting a light in the initial listing phase so it seems unlikely they would subsequently sell their only light.

We now describe this system in greater detail: When a free light was distributed to a household head it came with an associated coupon card. This card included the name and date of birth of up to two household heads, the serial number of the light assigned to that household, the recharge price for that light, and the expiration date of the coupon (i.e., after three months). Consumers were also aware that the recharge prices were the result of a lottery. Qualitatively consumers were happy with the system and believed it to be fair. A card included 15 perforated coupons each with a unique coupon ID. At every recharge the household head would bring their government issue ID document and a coupon would be torn off and handed to the entrepreneur who was instructed to confirm the name and date of birth and light serial on the card matched the ID document and the light serial. Given a match the entrepreneur enters the specific coupon code into the Octopus charger and recharges the light. The light serial number, coupon code, and timestamp is then automatically transmitted to our cloud database and if there is a match the VLE is reimbursed the difference between the full price and the discounted price, therefore incentivizing honesty by all parties.

The coupons and thus experimental interventions ran for a total of 3 months (which we term Phase II, short-run demand). Thereafter pricing reverted to the standard recharge price of 100RWF per charge. We then further track usage rates for an additional 3 months (for a total study-length of 6 months; although we record data up to 9 months for robustness tests) to see the extent to which behavior persisted or decayed over time, even when the interventions had concluded (which we term Phase II, long-run demand).

The experiments in Ruhango were carried out in the same district but differed in location—specifically different sectors as Rwandan administrative units are called. In Rwanda, Districts > Sectors > Cells > Villages. We do not include maps showing individual villages since this could identify households and Nuru's, a for-profit's, operations precisely. Graphic 1 below, however, displays a flowchart and table which outlines the village selection, experimental design, and timeline for both field experiments.

Graphic 1: Details of experimental design



	Experiment 1		Experiment 2
	Huye: 6	*Ruhango: 12*	*Ruhango: 29*
Randomized Trial	Upfront Price Vouchers		User Fee Recharge Coupons
Stratification	Prices in fixed proportions per village		Prices in fixed proportions per village
Upfront prices	Priced		Free
User fees	Full user fees 3 months		Discount coupons 3 months
User fees	Full user fees 3 months		Full user fees 3 months
Track Usage for total	6 months		6 months
Follow up surveys			GPS survey 29 villages
			Customer survey in subset of 12 villages in Ruhango limited by budget 1000 HHs

3.2 Data

There are five main sources of data utilized in this paper. We outline these here and break each down specifically for the two randomized trials or phases. The Phase I sample includes 1987 households from 18 villages and has two sources of data. First, we collect pricing voucher (which varied the upfront price of lights) information for each household in this sample. To do so we conduct a census of every village and collect data on the price each household faced and whether a household purchased a light, household identifier, the light serial number and the recharge-center serial number as well as village and district information. From this dataset we are able to tell how take-up or initial demand depends on the upfront price or fixed cost of lights (Phase I).

Second, we also develop a purpose-designed data collection technology to measure long-term usage patterns remotely minute-by-minute via Global System for Mobile communications (GSM) technology. Lights are charged at the village recharge center. Therefore, to objectively measure usage statistics, individual lights were programmed to communicate with the recharge-center and the recharge-center engineered to communicate via GSM to our cloud-based database. At each recharge the light transmitted its serial number to the charger. The charger then recorded a date-time stamp, accurate to the minute, as well as the amount of charge delivered to the light, and the length of time on charge. All this information was then immediately transmitted via GSM to our cloud-based database. Thus, we have data on both the extensive margin, whether a household used a light or not, as well as the intensive margin, how much a household used a light giving us a richer picture of long-term usage patterns. We thus have big data on behavior for each household in our sample, and our data is objective in that it does not suffer from many of the weaknesses of surveys such as social desirability bias and experimenter demand effects (Zwane et al., 2011). This data, combined with the pricing voucher data above, allow us to link upfront initial price paid to long-run usage, or recharge frequency (Phase I).

For the Phase II experiments, in a sample of 2867 households from 29 villages, we collected data from four sources (this sample is separate from Phase I which was in a different location). First, we collect pricing coupon data (which varied the recharge price or user fee). Here we recorded, for each household: the recharge price, or coupon value, coupon codes, household identifier, names and personal ID numbers of household heads, light and recharge-center serial numbers, and village and district information.

Second, as in Phase I, we implement GSM data collection technology in this sample to objectively measure light usage behavior. The recharge coupon pricing data combined with the GSM usage or recharge frequency data, allow us to analyze the impact of randomized short-run user fee subsidies, a free trial in the case of zero user fees, on both short-run demand when the subsidies were active (Phase II short-run demand) and long-run demand after the subsidy period is over (Phase II long-run demand). Third, we also collect Global Positioning Systems (GPS) location data, as well as the location of each solar recharge station and local shop where alternative light sources are available. Since recharging requires visiting the recharge center, the distance a household is from this center is a measure of the inconvenience faced by households in using their lights and could be an important factor driving usage (we show this to be the case in separate work), and, therefore, its inclusion in our models would increase the precision and reliability of our estimates and we include this variable in robustness and balance tests.

Fourth, in a subset of 12 of the 29 villages from Ruhango, and a sample of 1000 households, we collect detailed follow-up data on every customer household in the village. This customer household survey captures characteristics of households including demographics, income, energy usage and expenditures, as well as questions designed to measure the internal validity of our experiments and the role of information constraints and other factors on consumer demand. Surveys were collected electronically, with a number of consistency checks and audits as implemented by our field partner, Innovations for Poverty Action (IPA), in all its field experiments. IPA staff, in collaboration with the research team, created a data quality assurance plan and materials before launching the surveys. This plan lays out in detail the requirements for back checks, high frequency checks, accompaniments, and spot checks. The surveys were bench-tested (in the office) and piloted (in the field) prior to launching. Field supervisors and a research coordinator accompanied the survey teams.

4 Experimental results

4.1 Phase I, short-run: The impact of upfront price on initial adoption

The Phase I randomized experiments focus on how short-run adoption depends on the upfront, or short-run, price of LED lights. To study this, we randomly vary the upfront price faced by consumers. The experimental results are shown graphically in Figure 1. Phase I adoption decisions are heavily influenced by own-price: initial take-up, or adoption, of LEDs is highly price elastic. The figure plots the share of households purchasing at each randomized price level (without regression adjustment with control variables). At zero price adoption is over 90 percent while at the full price of 3000RWF ($10 PPP) take-up is approximately zero percent.

Figure 1

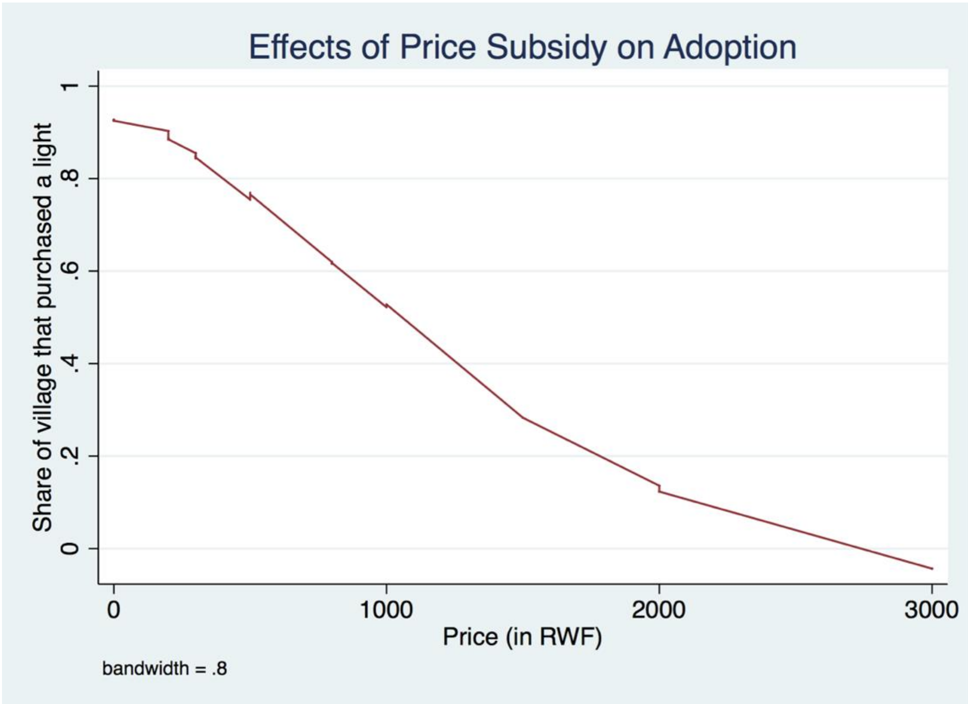


Table 1 presents the results of regressing *purchased*, a dummy variable equal to one if households purchased a light, on the randomized price point dummies. We, therefore, estimate a linear probability model where the omitted category is zero price and each coefficient represents the percentage point reduction in demand compared to demand when the price is zero. We estimate the following equation:

$$Y_{iv} = \beta_0 + \sum_{j=200}^{j=3000} \beta_j \, P_{ivj} + \gamma Village_{iv} + \varepsilon_{iv} \qquad (1)$$

where "Y_{iv}" is the outcome variable, in this case a dummy variable = 1 if a household purchased light *i* in village v. P_{ivj} is a treatment indicator variable which takes the value of one if light *i* was randomly assigned price *j* in village v. *j* includes prices 200, 300, 500, 800, 1000, 1500, 2000, and 3000RWF; "*Village*" is a vector of village fixed effect indicator variables. By including village fixed effects we correct for stratification, done at the village level, and increase the precision of our estimates in the above equations (Bruhn & McKenzie, 2009). ε_{iv} is the idiosyncratic error term. Given that treatment is allocated at the individual level we do not cluster our standard errors following recent developments in the econometric literature on clustering (Abadie et al, 2017), but instead report robust standard errors. We also include specifications which more cautiously allow for correlation within villages by clustering standard errors at this level.

Table 1 – Upfront Price and Demand

	Outcome Variable: Purchased			
	Household level	Clustered at village level	Village Fixed Effects	Village Fixed Effects clustered at village level
	1	2	3	4
Prices				
p200	-0.036*	-0.036	-0.020	-0.020
	(0.021)	(0.024)	(0.021)	(0.019)
p300	-0.019	-0.019	-0.051**	-0.051*
	(0.024)	(0.028)	(0.024)	(0.026)
p500	-0.155***	-0.155***	-0.171***	-0.171***
	(0.026)	(0.028)	(0.026)	(0.027)
p800	-0.251***	-0.251***	-0.284***	-0.284***
	(0.046)	(0.079)	(0.043)	(0.072)
p1000	-0.444***	-0.444***	-0.453***	-0.453***
	(0.035)	(0.072)	(0.034)	(0.069)
p1500	-0.700***	-0.700***	-0.732***	-0.732***
	(0.043)	(0.077)	(0.042)	(0.077)
p2000	-0.841***	-0.841***	-0.857***	-0.857***
	(0.025)	(0.037)	(0.025)	(0.037)
p3000	-0.892***	-0.892***	-0.879***	-0.879***
	(0.027)	(0.028)	(0.033)	(0.035)
Village Fixed Effects	No	No	Yes	Yes
Constant Intercept	0.891***	0.891***	1.092***	1.092***
	(0.018)	(0.053)	(0.031)	(0.023)
Observations	1,987	1,987	1,987	1,987
R-squared	0.403	0.403	0.451	0.451

*Notes: T*he table reports the coefficients, and robust standard errors in parentheses, from OLS regressions - linear probability models - of the dependent variable, purchased, on the randomized price points. Standard errors reported are robust in columns 1 and 3 given randomization occurred at the household level. Regression 2 more cautiously clusters at the village level which allows for correlations within villages. Given randomization was stratified on village, regression 3 includes village fixed effects. Regression 4 is the most cautious allowing for correlation at the village level by clustering standard errors at this level while also controlling for village fixed effects.

*** p<0.01, ** p<0.05, * p<0.1

If Y is demand then β_0 gives the average demand, or probability of purchase, in the reference village when the price is 0. β_{200} through β_{3000} are the coefficients of primary interest: they give the percentage point reduction in demand or take-up for each randomly assigned price treatment. Given randomization, these coefficients can be unbiasedly and consistently estimated by Ordinary Least Squares (OLS)—which is preferable given well-documented good properties including fewer distributional assumptions, so long as heteroscedasticity is corrected for via the use of robust standard errors (Angrist and Pischke, 2009). We also estimate probit and logit models as robustness tests (not shown). The marginal effects are broadly the same as OLS.

As is evident from column 4 all prices, except the lowest price of 200RWF, are statistically significant and economically large. When a positive price of 300RWF is charged demand falls by 5.1 percentage points relative to when a zero price is charged. At 500RWF demand drops more significantly by 17.1 percentage points. When 1000RWF is charged demand falls by 45 percentage points. Each price increase reduces demand further and is statistically significant with the highest price of 3000RWF ($10 PPP) reducing demand by 88 percentage points.

Therefore, demand is highly price-sensitive with barely any households purchasing lights at full price indicating that households are credit, saving, or liquidity constrained, or that the majority of households do not value LEDs as much as their market price. The finding that price is a significant factor in investment in solar LEDs, or more generally a preventive health product, is in keeping with the literature. In particular, results are in line with those from insecticide treated bednets in Cohen and Dupas (2010) and Dupas (2014a, 2014b), medications in Fischer et al. (2016) and deworming pills in Kremer and Miguel (2007). Moreover, Meredith et al. (2013) in the context of multiple health products finds 78% of demand is driven by prices alone and the authors argue their results imply the importance of subsidization. Our findings provide further support for the subsidization of health products generally, and more specifically, low-cost solar LEDs.

It should be noted that these effects are an upper bound on the true effect of prices on demand. This is because it is possible that households were especially price sensitive because they were aware they were receiving different prices via the voucher system. Although there was only a day or two between receiving the vouchers and when they were redeemed, it is certainly possible households discussed the prices they received with their neighbors. It is also possible that households were nudged into purchasing by the fact that the vouchers had an expiration date, or that liquidity constraints for those facing the high prices may have reduced demand and thus increased the elasticity measures.

4.2 Phase I, long-run: Impacts of upfront pricing on subsequent long-run usage

We have established that a zero or low upfront pricing strategy is required if high take-up is to be achieved. However, what are the long-term implications of such a strategy on usage of lights? Is long-term usage (in the context of this study 6 months), with a user fee attached, lower for households that paid zero or substantially reduced upfront prices? In other words, does charging an initial positive price screen out those who do not need LEDs, i.e. those who will not use them? Do initial prices act as a signal for how much consumers value the good leading to higher usage by those that paid a nonzero price?

This is not just a theoretical question since many actors in development argue that positive prices should be charged since users will not sufficiently use goods if they are given away for free (Cohen & Dupas, 2010). It is even more important in our setting where long-run usage carries a fee, so that initial willingness to pay might predict ability to pay overtime. We analyze this question using the data on the randomized upfront price as well as long-term usage patterns (6 months) remotely captured minute-by-minute via GSM technology. In Table 2, using OLS, we regress long-term usage, or recharge frequency per light, on upfront price paid controlling for village fixed effects and computing robust standard errors as the level of randomization is the household. We estimate the following equation:

$$Y_{iv} = \beta_0 + \beta_1 Price_{ivj} + \gamma Village_{iv} + \varepsilon_{iv} \tag{2}$$

where "Y_{iv}" is usage, or the recharge frequency (or the inverse hyperbolic sine transformation of recharge frequency), for light *i* in village *v*. Recharge frequency is the number of times a light was recharged over the first 6 months after distribution of lights. $Price_{ivj}$ is the upfront price level a household faced and takes the following values: 0, 200, 300, 500, 800, 1000, 1500, 2000, and 3000RWF; "*Village*" is a vector of village fixed effect indicator variables. We control for village fixed effects because random assignment was stratified by village. ε_{iv} is the error term. β_1 is the coefficient of primary interest: it gives the impact of upfront price paid on subsequent usage. Note that here the sample size is reduced because it only consists of households that purchased a light (whether at zero price or any positive price). It is only within this sample of households that usage can be defined and tracked. Column 1 presents the results with the outcome variable in level form. For an alternative interpretation and as a robustness test, column 2 present results with the outcome variable transformed using the inverse hyperbolic sine transformation (IHS). It thus gives an approximate percent effect of price on usage. We use the IHS transformation instead of the natural logarithm, because recharge frequency, or usage, has a significant number of zero values (Burbidge et al., 1988). This is becoming standard practice in the empirical literature (for example, see the influential paper by Haushofer and Shapiro (2016)).

Table 2 - Impact of Free Lights on subsequent Usage

	Outcome Variable: Recharge Frequency Per Light	
	Level	*In Hyp Sin*
Purchase Price	0.109	-0.014
	(0.322)	(0.058)
Village Fixed Effects	YES	YES
Observations	1,377	1,377
R-squared	0.171	0.140

Notes: The table reports the coefficients, and robust standard errors in parentheses, from OLS regressions where the dependent variable is usage or recharge frequency per light. The experiments were stratified on village so all regressions include controls for village fixed effects.

*** p<0.01, ** p<0.05, * p<0.1

Upfront price has no overall statistically significant effect on subsequent long-term usage rates, over a 6-month period. This is contrary to expectations if price paid were to signal how much a consumer would subsequently use a good. The coefficients are also small and differ in sign. Table 3 digs more deeply into this relationship by regressing recharge frequency, or *usage*, on individual randomized price points paid by consumers. We estimate the following regression by OLS:

$$Y_{iv} = \beta_0 + \sum_{j=200}^{j=3000} \beta_j \, P_{ivj} + \gamma Village_{iv} + \varepsilon_{iv} \qquad (3)$$

where "Y_{iv}" is the recharge frequency (or IHS of recharge frequency) for light *i* in village v. P_{ivj} is a treatment indicator variable which takes the value of one if light i was randomly assigned price *j* in village *v*. *j* includes prices 200, 300, 500, 800, 1000, 1500, 2000, and 3000. If Y is recharge frequency then β_0 gives the average usage in the reference village when the price is 0. β_{200} through β_{3000} are the coefficients of interest: they give the change in usage for each randomly assigned price treatment. All but two of the eight coefficients are negative and only one price is robustly significant: having paid the highest price 3000RWF is associated with a reduction in long term usage relative to those that paid zero price.

Table 3 - Impact of free lights on subsequent usage

	Outcome Variable: Recharge Frequency	
	Level	*In Hyp Sin*
	1	2
Prices		
p200	-0.376	-0.144**
	(0.302)	(0.065)
p300	-0.044	-0.093
	(0.314)	(0.074)
p500	-0.053	-0.077
	(0.333)	(0.065)
p800	-0.041	-0.042
	(0.411)	(0.098)
p1000	-0.325	-0.120
	(0.516)	(0.092)
p1500	0.721	0.127
	(0.734)	(0.179)
p2000	1.301	0.064
	(1.627)	(0.237)
p3000	-2.774***	-0.309***
	(0.495)	(0.088)
Village Fixed Effects	YES	YES
Constant Intercept	2.037***	0.889***
	(0.298)	(0.083)
Observations	1,377	1,377
R-squared	0.175	0.145

Notes: The table reports the coefficients, robust standard errors in parentheses, from OLS regressions where the dependent variable is usage or recharge frequency per light. All standard errors are robust and the level of randomization is the household. The experiments were stratified on village so all regressions include controls for village fixed effects.

*** p<0.01, ** p<0.05, * p<0.1

This is strong evidence against the hypothesis that paying for a light means a household uses a light more. This result is in line with the development economics' literature including Cohen & Dupas (2010), Dupas (2014a), who find households given antimalarial bednets for free still valued and used them, but not with Ashraf et al (2010), in the context of water chlorinators who find charging a positive price leads households to subsequently use the good more, or the evidence from lab and scanner data, for example Kalyanaram and Little (1994) who find reference effects of prices. This could also be evidence of the presence of severe liquidity constraints: households are simply unable to afford to pay lump sum prices for lights, yet when they receive them they do use them even where use carries a user fee.

4.3 Phase II: The impact of short-run usage subsidies on short and longer-run use in the context of repeated purchases

We have presented evidence that subsidies or a low upfront price are required if high take-up is to be achieved and that the long-run implications are that households still, pay to, use lights even if they paid zero upfront prices. We now turn to examining consumer behavior after a light is already owned and consumers must pay a small PAYG user fee for repeated purchases (i.e., to recharge their light). We ask, how do short-run subsidies, or zero prices, affect both short-run adoption as well as long-term demand in the context of repeated use carrying a user fee? This is an important and highly contested question in the realm of development products for the ultra-poor.

The current literature only focuses on two purchases: an initial purchase where the price is randomly varied and a second subsequent purchase, which then allows estimation of the impact of the initial price paid on *additional* purchases. In this context Dupas (2014a) finds short-run subsidies actually increase long-run demand of insecticide treated bednets and argues this is due to positive learning about the value of the product that a subsidy provides, while Fischer et al. (2016) find the reverse: short-run subsidies decrease long-term demand of three medications and the authors argue this is due to anchoring on short-run prices. We extend this small literature by focusing on multiple purchases and a new product: rechargeable LEDs carrying a user fee. In this context, do short-run subsidies, or a three-month trial of reduced prices, increase or decrease long-run demand?

4.3.1 Phase II, short-run: The impact of short-run usage subsidies on short-run repeated use

First, however, we ask, how do short-run subsidies affect short-run demand or paid usage in the context of repeated purchases with micro user fees? To answer this question, we randomly vary the user fee faced by consumers via the random assignment of discount coupons valid for a period of three months and record the usage of lights, or the number of times a light is recharged. Seven prices were randomized: 0, 50, 60, 70, 80, 100, and 120RWF (This translates to USD$0 – $0.40 Purchasing Power Parity (PPP) per recharge).[5] We check how effective randomization was at balancing covariates. We do not have consumer pre-intervention data so we are unable to carry out broader balance tests. However, linear distance from the recharge center computed via GPS is equivalent to a fixed baseline characteristic in that it is very unlikely to be affected by subsequent treatment. In other forthcoming work we show that distance is a significant predictor of light usage so it is a particularly important variable to examine. In Table A2 in the appendix we regress 'treatment', or each price dummy, on village dummies and linear distance from the recharge center. Distance and village do not predict any price dummy indicating that randomization is balanced across this important baseline characteristic--a driver of usage, distance to the recharge center. We next estimate the following equations:

$$Y_{iv} = \delta_{100} + \sum_{j=0}^{j=120} \delta_j P_{ivj} + \gamma Village_{iv} + \varepsilon_{iv} \tag{4}$$

where "Y_{iv}" is the outcome variable in this case the recharge frequency for light *i* in village *v*. P_{ivj} is a treatment indicator variable which takes the value of one if light *i* was randomly assigned *j* recharge price in village *v*. *j* includes prices 0, 50, 60, 70, 80, 120RWF "*Village*" is a vector of village fixed effect indicator variables. We again control for village fixed effects because these experiments were also stratified by village. Given that treatment is allocated at the household level, we do not cluster and instead report robust standard errors (columns 1 and 2) but we also include

[5] Rwanda PPP in 2017 is 305.71. Available from the World Bank at: https://data.worldbank.org/indicator/PA.NUS.PPP

specifications which more cautiously allow for correlation within villages by clustering standard errors at this level (columns 3 and 4). As is evident from table 4, the levels of statistical significance do not change much when we cluster at the village level. We also report specifications with recharge frequency transformed using the IHS transformation as robustness tests. We prefer to report OLS results as our primary specifications because of the well-documented good properties of OLS, including fewer distributional assumptions, and to facilitate interpretation. Additional models, not shown, include poisson count, tobit censured, and zero inflated poisson regressions (given recharge frequency has a significant number of zeros). Out of the additional estimation methods, the tobit censured regression model fits the data best according to Akaike and Bayesian Information Criteria. Results are qualitatively the same as OLS.

We report the control group mean which had a price of 100RWF. δ_0 through δ_{120} are the coefficients of interest: they give the change in recharge frequency for each randomly assigned price treatment relative to the control of 100RWF. Table 4 presents the results of regressing recharge frequency, or usage, on the individual randomized usage fees paid by consumers using OLS. In the short-run, or the three months that the discount coupons were active, most interventions are statistically and economically significant. Usage is highly price-sensitive: The precise increase in recharges for price 0, δ_0, in column 1 of Table 4, is 2.8 recharges which is a 156 percent increase in usage, or demand, relative to the control of 100RWF. Continuing interpretation of columns 1: charging a price of 50RWF increases usage by 67 percent; and 60RWF also causes a statistically significant increase in usage or demand as does price 70 and 80, but not 120RWF which is negative and not significantly different from 100RWF.

Table 4 - Impact of short-run user subsidies on short-run usage

	Outcome Variable: Recharges per light			
	Level	In Hyp Sin	Level	In Hyp Sin
	1	2	3	4
Price 0	2.802***	0.833***	2.802***	0.833***
	(0.225)	(0.067)	(0.420)	(0.106)
Price 50	1.190***	0.408***	1.190***	0.408***
	(0.204)	(0.070)	(0.234)	(0.074)
Price 60	0.712***	0.250***	0.712***	0.250***
	(0.195)	(0.071)	(0.233)	(0.089)
Price 70	0.492**	0.158**	0.492**	0.158*
	(0.195)	(0.071)	(0.230)	(0.088)
Price 80	0.325*	0.120*	0.325	0.120
	(0.183)	(0.069)	(0.244)	(0.088)
Price 120	-0.060	-0.070	-0.060	-0.070
	(0.190)	(0.072)	(0.183)	(0.081)
Village Fixed Effects	Yes	Yes	Yes	Yes
Control group mean	1,794	0,752	1,794	0,752
	[2.295]	[0.719]	[2.295]	[0.719]
Observations	2,867	2,867	2,867	2,867
R-squared	0.302	0.331	0.302	0.331

Notes: The table reports the coefficients, and robust standard errors in parentheses, from regressions of the dependent variable, recharges per light, on randomized treatment indicators and village fixed effects, since randomization was stratified on village. The reference category is the control group where price was randomly assigned at 100 francs per recharge. The control group mean and standard deviation, in brackets are also presented for comparison purposes. Randomization was carried out at the household level so we report robust standard errors in columns 1 and 2. Even though randomization was done at the household level, in columns 3 and 4 we cautiously allow correlation at the village level by clustering our robust standard errors at this level.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

We conclude this section by noting that in the short-run repeated use is highly elastic, just as initial demand is very elastic with respect to the upfront price of lights. Therefore, to increase adoption a reduced pricing strategy or subsidies may be required.

4.3.2 Phase II, long-run: The impact of short-run usage subsidies on longer-run demand or repeated use

Next, we study what happens to long-run demand after the short-run subsidization, or the free trial, three-month period is over. Recent research shows that incentive-based behavioral interventions can work. Most relevant is that they can have long lasting effects even beyond the treatment period (Mochon et al., 2017). A free trial, or short-run subsidies, can be thought of as a price incentive aimed at cementing a new welfare improving and emissions reducing habit. Another hypothesis, popular in the literature, is that free trials serve as an intervention which increases positive learning: a free trial can remove information frictions because it allows uninformed consumers to use the product and learn its benefits over existing alternatives, which leads to higher use and willingness to pay in the long-run (Dupas, 2014a). This literature motivates the design of our free trial intervention which entails providing lights free upfront along with coupons for free recharges for the first three months after distribution.

In this section we study the long-run effects of this intervention (at 6 months and 9 months). Table 5 presents results from estimating the same equations as used for the short-run impacts, 4 and 5 above, but this time on post-intervention data after the expiration of the coupons and when the recharge fee was uniformly set to full price or 100 RWF. We do this at 6 months and 9 months after the beginning of the study (i.e., 3 months and 6 months of post-subsidy data). Results do not differ so we present those from 6 months here. We examine this data to see if long-term recharge frequency remains higher in the zero-priced and other recharge fee interventions, i.e. whether habit formation and positive learning lead to higher long-run demand, or whether the effect of anchoring on the initial subsidized prices leads to lower long-run demand. Table 5 row one, column 1, presents the most important result: households that were randomly assigned a zero priced coupon for three months recharged 133 percent more, over a three-month period *after* the price was increased to 100 RWF, than households that were initially assigned the full price of 100 RWF. This result is highly statistically significant and practically meaningful. Furthermore, households that were assigned a 50 RWF coupon also paid to use their lights more: approximately 71 percent over the post-intervention period.

Table 5 - Long Run Impact of Short Run Subsidies

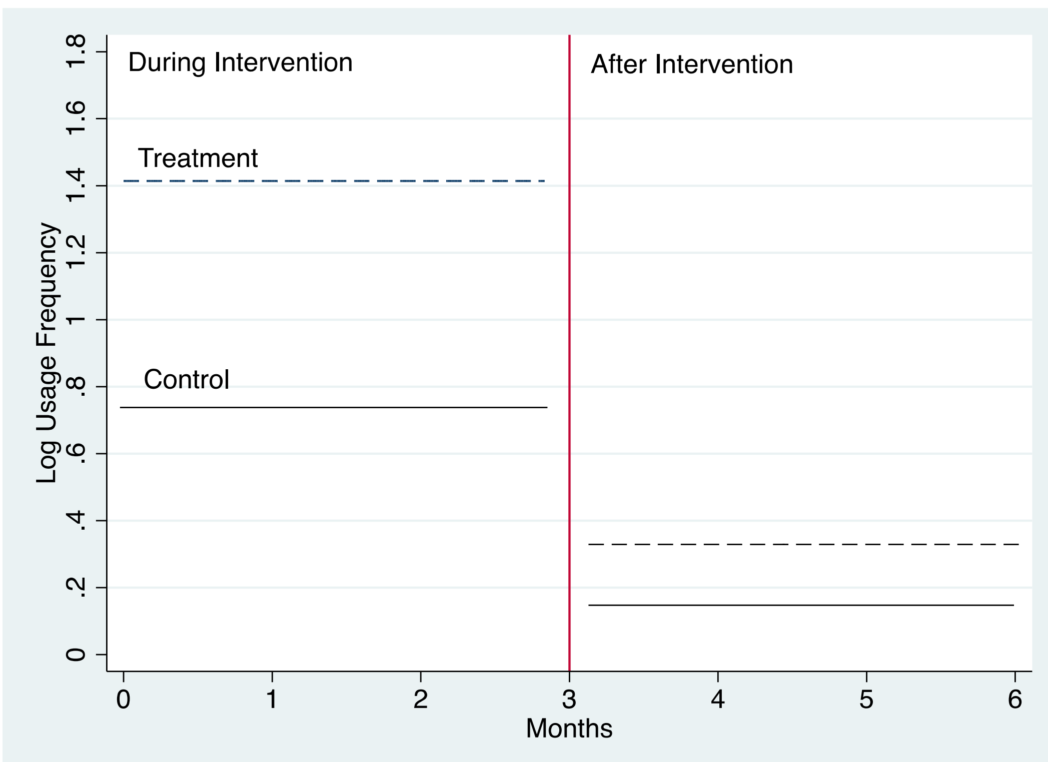
	Outcome Variable: Recharges per light			
	Level	In Hyp Sin	Level	In Hyp Sin
	1	2	3	4
Price 0	0.343***	0.147***	0.343*	0.147**
	(0.107)	(0.044)	(0.192)	(0.065)
Price 50	0.179**	0.088**	0.179	0.088
	(0.088)	(0.040)	(0.107)	(0.052)
Price 60	0.207**	0.078*	0.207	0.078
	(0.103)	(0.042)	(0.160)	(0.060)
Price 70	-0.021	0.001	-0.021	0.001
	(0.074)	(0.038)	(0.087)	(0.040)
Price 80	0.145	0.039	0.145	0.039
	(0.097)	(0.040)	(0.137)	(0.053)
Price 120	0.105	0.026	0.105	0.026
	(0.090)	(0.040)	(0.088)	(0.036)
Village Fixed Effects	Yes	Yes	Yes	Yes
Control group mean	0,254	0,128	0,254	0,128
	[0.909]	[0.359]	[0.909]	[0.359]
Observations	2,867	2,867	2,867	2,867
R-squared	0.181	0.192	0.181	0.192

Notes: The table reports the coefficients, and robust standard errors in parentheses, from regressions of the dependent variable, recharges per light, on randomized treatment indicators and village fixed effects, since randomization was stratified on village. The reference category is the control group where price was randomly assigned at 100 francs per recharge. The control group mean and standard deviation, in brackets are also presented for comparison purposes. Randomization was carried out at the household level so we report robust standard errors in columns 1 and 2. Even though randomization was done at the household level, in columns 3 and 4 we cautiously allow correlation at the village level by clustering our robust standard errors at this level.

*** p<0.01, ** p<0.05, * p<0.1

This can also be shown graphically over time as in figure 2: during the three months of the intervention period usage is significantly higher for households that received the free trial. Over a six-month period, and after full user fees were introduced, usage continued to be substantially higher in the treatment even when there were no other differences between the groups other than whether they had been exposed to a free trial or reduced user fees. We note that there is a drop off in the number of recharges recorded over time due to GSM transmission equipment failure in some villages. Given this, we also estimate results in Table 5 on various restricted samples which include only villages with a significant number of recharges recorded. The magnitude of the impact of subsidies in this smaller sample either stays the same or declines somewhat, from 133 percent to 131 percent and remains statistically significant. However, the most restricted sample throws out a large amount of data, so we report these results in the appendix only (Table A2). It is also important to note that given the experiments are stratified on village, so prices faced by consumers are uncorrelated with village, failure in transmissions at the village level will not bias results found in other villages.

Figure 2: The effect of short-run subsidies over time



Demand or usage, is not complete even when user fees are zero. Therefore, factors other than price must also be driving take-up and long-term adoption. One possibility is the inconvenience costs associated with traveling to the village-level centralized solar recharge center. To increase the precision and validity of our estimates we collect GPS data on the distance of each household from the centralized recharge center, and we control for this proxy in robustness tests which we present in the appendix. We study the impact of inconvenience in more detail in forthcoming work. Here we note that including distance as a covariate marginally reduces the size of the price coefficients. For instance, the coefficient on the impact of the free trial intervention is .348 before and .337 after inclusion of distance in our model (Table A1 in the Appendix; these numbers represent the number of additional recharges in the post-intervention period). Moreover, the coefficient on price 50 does not change. Therefore, we can be confident our main results are not biased because the price treatments are orthogonal to inconvenience. Thus, in the context of repeated use with a user fee, demand is higher in the long-run after short-run subsidies, or a free trial, than after status quo pricing.

5 Conclusion

Available evidence shows that off-grid rechargeable solar LEDs save consumers on lighting expenditures over time, have longer lifespans, and are of higher quality than alternatives. They can also be expected to have health and environmental impacts. We show, however, that rural households fail to invest in them unless upfront prices are close to zero. Without targeted support households, therefore, fail to optimize. We implement pricing interventions designed to address liquidity constraints, de-bias consumers by removing information frictions allowing them to experience the benefits of the technology, as well as directly nudge them onto a path with higher long-term payoffs. This study estimates demand curves for both the initial price of low-cost LEDs as well as the subsequent user fee for repeated purchases, while also estimating the impact of an intervention to increase adoption, short-run subsidies, or a free trial, on longer-run demand. Overall, our results identified important barriers to take-up of clean LED lighting, particularly cost—both upfront and user fee prices are too high—while lack of experience using the technology also leads to under investment.

Specifically, we find that, first, initial demand for lights is extremely price sensitive with barely any households purchasing at full price and over 90 percent doing so when the upfront price is 0. This has strong implications for take-up and consequently successful business or non-profit distribution models. This finding is in line with the literature on pricing health goods as well as electricity and solar LEDs.

Second, using variation in upfront price paid and objective data on long-term usage we show subsequent long-term usage rates of LEDs does not depend on the initial price paid, even when usage is not free – i.e., this is in the context of user fees for repeated use. Thus, initial price paid does not act as a signal for how much a customer will subsequently use the good -- a standard assumption in economic theory (Ashraf et al., 2010). This finding is in accordance with our observational evidence on low-cost LEDs and experimental evidence on insecticide treated bednets in Cohen and Dupas (2010) and Dupas (2014a), deworming pills in Kremer and Miguel (2007), shoes to prevent worm infections in Meredith et al. (2013) and a range of general health products (Dupas, 2014b), water chlorination in Ashraf et al. (2010) or a number of medications in Fischer et al. (2016).

This evidence provides a rationale for subsidies for the upfront price of lights, but also for a reduced upfront pricing strategy for a for-profit where flexible PAYG user fee schemes are possible. If subsidies are infeasible, lights should be sold for free upfront, with pay-as-you-go PAYG user fees recouping costs over the longer term. However, third, we show that long-run use is also highly elastic with respect to these user fees. Therefore, even charging low rates, such as USD$0.15 PPP per recharge, will reduce adoption and use substantially making subsidies necessary if complete adoption is to be achieved. A complete motivation for subsidies would include a cost-effectiveness analysis from a large-scale randomized welfare evaluation of this intervention.

Fourth, we present a policy-relevant intervention which does increase adoption and long-run usage: Short-run subsidies, or a free trial period with user fees set to zero for 3 months, has a significant positive impact on long-run usage after the free trial is discontinued. It does so at 3 and 6 months after the removal of zero pricing (or 6 and 9 months in total). Therefore, giving people an opportunity to use the lights for free for an extended period increased the likelihood they would use them when full-price user fees were later charged. This is an intervention which for-profit firms can make use of immediately as it is profitable, but it also provides an additional rationale in favor of short-run subsidies.

Clearly, there is no one size fits all model in terms of pricing products for the poor. Results differ depending on the product and context, with our findings being more in line with Dupas (2014a) but differing from those of Fischer et al (2016).

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Appendix

A1. Robustness tests: controlling for inconvenience with travel distance

Long-run demand or usage, is not full even when user fees are zero. Therefore, factors other than price must also be driving take-up and long-term adoption. One possibility discussed in the main text is the inconvenience costs associated with traveling to the village-level centralized solar recharge center. To increase the precision and validity of our estimates we collect GPS data on the distance of each household from the centralized recharge center, and we control for this proxy in robustness tests. We study the impact of inconvenience in more detail in forthcoming work.

Table A1 below presents the results (column 1 and 3 represent the number of additional recharges in the three-month post-intervention period). The first two columns include the outcome in level form and IHS in the sample for which we have GPS data. The second two columns are the same but include distance as a covariate. Distance is statistically significant but the coefficient is small and the coefficients on the price treatments barely change. For instance, the coefficient on the impact of the free trial intervention is .348 before and .337 after inclusion of distance in our model. For price 50 the coefficients do not change at all. Therefore, we can be confident that the price treatments are orthogonal to distance and results are not significantly biased.

Table A1 - Results after controlling for distance

	Outcome Variable: Recharges per light			
	Without distance		With distance	
	Level	In Hyp Sin	Level	In Hyp Sin
	1	2	3	4
Price 0	0.348***	0.137***	0.337***	0.131***
	(0.122)	(0.048)	(0.121)	(0.048)
Price 50	0.179*	0.085**	0.179*	0.085**
	(0.096)	(0.042)	(0.096)	(0.042)
Price 60	0.257**	0.100**	0.252**	0.097**
	(0.115)	(0.045)	(0.114)	(0.045)
Price 70	-0.020	0.002	-0.020	0.002
	(0.080)	(0.040)	(0.079)	(0.040)
Price 80	0.141	0.046	0.129	0.040
	(0.102)	(0.043)	(0.101)	(0.042)
Price 120	0.053	-0.002	0.049	-0.004
	(0.095)	(0.042)	(0.095)	(0.042)
Distance			-0.000***	-0.000***
			(0.000)	(0.000)
Village Fixed Effects	Yes	Yes	Yes	Yes
Control group mean	0.254	0.128	0.254	0.128
	[0.909]	[0.359]	[0.909]	[0.359]
Observations	2,500	2,500	2,500	2,500
R-squared	0.188	0.197	0.194	0.205

Notes: The table reports the coefficients, and robust standard errors in parentheses, from regressions of the dependent variable, recharges per light, on randomized treatment indicators and village fixed effects, since randomization was stratified on village. The reference category is the control group where price was randomly assigned at 100 francs per recharge. The control group mean and standard deviation, in brackets are also presented for comparison purposes. Randomization was carried out at the household level so we report robust standard errors in columns 1 and 2. Even though randomization was done at the household level, in columns 3 and 4 we cautiously allow correlation at the village level by clustering our robust standard errors at this level.

*** p<0.01, ** p<0.05, * p<0.1

Table A2 - Balance Tests

	Outcome Variables: Randomized Price Treatments						
	Price 0	*Price 50*	*Price 60*	*Price 70*	*Price 80*	*Price 100*	*Price 120*
Distance	-0.000	0.000	-0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,500	2,500	2,500	2,500	2,500	2,500	2,500
R-squared	0.00300	0.00109	0.00165	0.00194	0.00215	0.00145	0.00147

Notes: The table reports the coefficients, and robust standard errors in parentheses, from regressions of the dependent variables, randomized treatment indicators, on village fixed effects and linear distance to the recharge center computed via GPS.

*** p<0.01, ** p<0.05, * p<0.1

Table A3 - Long Run Impact of Short Run Subsidies - restricted sample

Outcome Variable: Recharges per light

	Level	In Hyp Sin
	1	3
Price 0	0.562***	0.198**
	(0.210)	(0.080)
Price 50	0.335*	0.165**
	(0.172)	(0.071)
Price 60	0.443**	0.158**
	(0.205)	(0.075)
Price 70	-0.026	0.006
	(0.137)	(0.067)
Price 80	0.259	0.094
	(0.186)	(0.075)
Price 120	0.092	-0.006
	(0.171)	(0.073)
Village Fixed Effects	Yes	Yes
Control group mean	0.42	0.27
	[1.8]	[0.5]
Observations	1,384	1,384
R-squared	0.172	0.197

Notes: The table reports the coefficients, and robust standard errors in parentheses, from regressions of the dependent variable, recharges per light, on randomized treatment indicators and village fixed effects, since randomization was stratified on village. The reference category is the control group where price was randomly assigned at 100 francs per recharge. The control group mean and standard deviation, in brackets are also presented for comparison purposes. Randomization was carried out at the household level so we report robust standard errors

*** p<0.01, ** p<0.05, * p<0.1

Table A4 - Household socioeconomic characteristics Huye	
Characteristics	
Household Demographics	
Total of household members	5.10
Number of children (<18years) in the household	2.64
Age Head of HH	48.40
Years of education of head of HH	4.40
Household welfare	
Household income per week	14793.68
Household income (log)	8.98
Number of phones in the household	0.78
Household savings in last month	706.79
HH Mother Working Dummy	0.24
HH Working Dummy (non-farming)	0.62
Total working HH Members	1.47
Average hours of sleep per night	9.03
Lighting expenditures and usage	
Total sources of light in household	1.65
Light time per week	16.46
Expenditure on light per week	248.41
Uses a dirty light source	0.52
Studying and Light	
Children's study hours per week	4.10
Study use of dirty light source	0.17
Individual labor market variables	
Working other than farming dummy	0.28
Work hours per week (other than farming)	6.67
Observations	824